**Review for *Spaceborne differential absorption radar water vapor retrieval capabilities in tropical and subtropical boundary layer cloud regimes* by *R. Roy et al***

The study assesses the capabilities of spaceborne Differential Absorption Radar (DAR) for tropospheric humidity profiling in typical planetary boundary layer (PBL) cloud conditions along the transition from sub-tropical to tropical regimes. Five different LES model cases are used to simulate spaceborne radar measurements at 155.5, 168 and 174.8 GHz. DAR capabilities are evaluated based on the simulated measurements for each scene regarding the retrieval of in-cloud water vapor profiles, sub- and above-cloud water vapor amount, as well as IWV in clear-air scenes. Application examples are given for deriving near-surface RH and in-cloud temperature profiles. The authors analyze expected uncertainties and instrument requirements and conclude that spaceborne DAR holds the potential of filling current observational gaps of PBL profiling.

The study presents a suitable and novel framework to address the current observational gap of PBL profiling. It is an important contribution to the field with exciting results, and particularly timely as DAR and G-band radar technique are advancing. The study is clearly written and well presented. It comprises a very detailed presentation of the applied forward simulator and retrieval algorithm method, as well as an extensive results section. The amount of material makes the manuscript quite long. I have some major and minor comments of what is otherwise a convincing manuscript.

***General comments:***

1. DAR aims at filling observational gaps in PBL profiling. The authors should emphasize which type of variability can be resolved by their approach, and if this meets the needed requirements to characterize the analyzed regimes.

   Thanks for pointing out that we neglected to place DAR within the context of a larger observing system. The NASA PBL Decadal Survey Incubation Study Team recently released a report (https://science.nasa.gov/earth-science/decadal-pbl) in which they envision DAR as only one piece of a comprehensive core observing system containing also a DIAL and advanced hyperspectral IR and microwave sounders. The objective of the DIAL/DAR is to observe the vertical variability while the passive sounders provide the horizontal variability. We have added some text to this effect.

   Added Text Page 2, Line 11 (after 200m): "The advantage of the active DIAL/DAR approaches is to constrain the vertical variability in water vapor desired by the decadal survey (NASEM, 2018). It is anticipated that these observations would exist within a larger observing system where the requisite horizontal variability is observed by passive infrared and microwave systems."

2. The use of a scale height for exponential humidity interpolation requires more justification. In trade wind conditions, the humidity profile strongly deviates from an exponential profile. The limitations should be further discussed and the impact of this assumption on the retrieval results should be quantified in more detail.

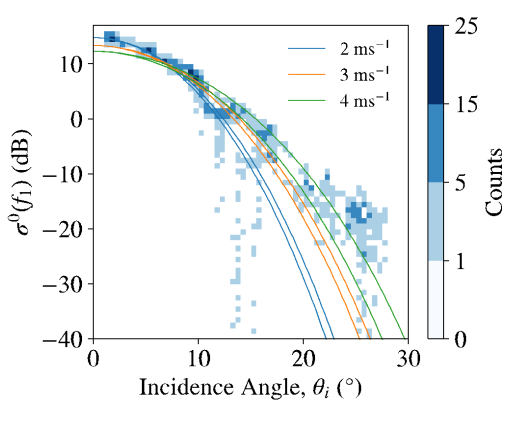
   Added text Page 11, Line 24 (after cross section). 'We note that the assumption of the exponential profile is not necessary. We use this assumption here to demonstrate how a radar-only retrieval might perform in the absence of any additional information. In practice, in an operational environment a more detailed profile shape would be taken from a weather analysis system or from coincident observations from passive sounders. In fact, we show below that the derived column water vapor measurements below are significantly sensitive to the exponential scale height motivating the future use of ancillary data in the DAR retrievals'.

We note to the reviewers that the sensitivity to the assumed scale height is shown in figures 6 and 8 for the RICO and BOMEX cases. We agree that the assumption of an exponential profile is often wrong, however we argue that in the absence of any other information (i.e. a DAR-only retrieval) an exponential scale height is the optimum general-scenario choice.

3. Intelligent scanning technique is a powerful tool for future satellite-based applications. When applying this sampling strategy, do the authors account for the tilted, off-nadir inclination angle and the resulting change of surface NRCS in their forward simulations? If not, a quantification of the impact should be added.

   The simulations do not account for the change in the NRCS, however these effects are negligible. We have a paper in review on the NRCS at G-band (see figure below). We envision that a 40-50 km cross-track sampling range would be sufficient to address most of the sampling challenges for DAR associated with shallow convection, and this regime is our motivation for pursuing this approach. From 400 km altitude this sampling capability can be accommodated with roughly +/-3 degrees of scanning. The variation in NRCS within this narrow range of incidence angle is well modeled by a geometric optics approach and is on the order of 1-2 dB. Variations due to wind speed will be larger than this. In any case, in nearly all scenarios the retrieval uncertainty will not be affected by these small changes in surface reflection because the precision is independent of signal magnitude in the regime SNR >> 1. In cases where the surface SNR is not much greater than 1 the signal is generally completely attenuated by precipitation and there is no visible surface reflection at all.

   Added text Page 13, Line 32 (after position) "We envision a cross-track scanning capability on the order +/- 3 degrees which from 400 km altitude provides ~42 km range in cross track sampling location. This relatively limited scanning capability would provide significant increases to cloud sampling opportunities in shallow convective cloud regimes while also limiting the technical complexity of a wide-angle scan. We note that we do not model the angular dependence of the NRCS in this study, which has limited sensitivity over this modest angular range (Roy et al., 2021)."




Observed NRCS over ocean from the airborne observations with VIPR over a very calm ocean surface in the Santa Barbara channel. Smooth curves show theoretical calculations using a geometric optics approach for various wind speeds and a Cox-Munk surface model. Note that the curves are very flat near nadir even in this very calm case and the sensitivity to angle decreases as wind speed increases. Finally, typical wind speeds over ocean are ~ 7 m/s

4. The authors add a third frequency to the standard 2-frequency DAR approach. More information should be provided regarding the choice of the channel. The authors should also highlight in which conditions and at which heights signal saturation occurs in one of the three channels, e.g. as function of hydrometeor and gas loading. Which conditions would be particularly favorable and which are most challenging for spaceborne DAR to measure in and fill existing observational gaps?

   The architecture is largely constrained by regulations rather than physics. 174.8 GHz is the closest frequency to the water line at which we are able to transmit because of regulations. The single best thing we could do to enhance performance would be to move that on-line frequency slightly closer to 183 - but we cannot. We point the reviewer to page 1, line 15, which states 'The 3 transmit frequencies located at 155.5, 168.0, and 174.8 GHz are carefully chosen to lie in bands that do not feature international transmission restrictions (National Telecommunications & Information Administration (NTIA), 2015)'. 155.5 GHz is chosen because it lies within a small piece of available spectrum between 151.5 and 155.5 GHz that is not designated as a 'passive' transmission band. We chose not to elaborate further on these details because they are far outside the scope of the typical radar scientist or atmospheric scientist.

   Added Text Page 3 Line 22: "The middle DAR channel used here differs slightly from the 167 GHz lower frequency channel used in past two-frequency DAR measurements with VIPR (Roy et al., 2020). This new location is chosen to minimize covariance between fit parameters in the three-frequency retrieval (see $a_2$ and $a_3$ in Eq. (6) and (7)).

5. The manuscript contains a lot of material, at times distracting the reader from the main messages. For example, the authors develop and present a radar forward simulator in detail as tool for their analysis. Why did they not consider an established tool such as CR-Sim (Oue et al., 2020; https://gmd.copernicus.org/articles/13/1975/2020/) or PAMTRA (Mech et al., 2020; https://doi.org/10.5194/gmd-13-4229-2020/)? Changes should be highlighted, also regarding the simulator presented in Millán et al., 2020 (https://doi.org/10.5194/amt-13-5193-2020). Many details on radar simulation are extensively described by literature.

   While these are useful tools, CR-Sim and PAMTRA both lack the ability to perform multiple scattering calculations for the radar, which is essential to capturing realistic radar propagation from a spaceborne platform at such high millimeter-wave frequencies. Furthermore, we don't believe that the existence of other radar simulators in the literature precludes the development of a custom simulator for a specific/targeted application or study. Indeed, it is still common practice to describe the ingredients of a millimeter-wave radar forward simulator without providing an exhaustive comparison to other such simulators already described in the literature. This can be found, for instance, in a recent paper on spaceborne radar simulation published in the same journal (Battaglia, A., Kollias, P., Dhillon, R., Lamer, K., Khairoutdinov, M., and Watters, D.: Mind the gap – Part 2: Improving quantitative estimates of cloud and rain water path in oceanic warm rain using spaceborne radars, Atmos. Meas. Tech., 13, 4865–4883, https://doi.org/10.5194/amt-13-4865-2020, 2020.)

   Thus, the authors should

(i) move technical details in Sec. 2.2. to the appendix;

We have moved the contents of section 2.2.1, which details the single-scattering calculations and microphysical parameterizations, to Appendix A. However, we believe sections 2.2.2 and 2.2.3 belong in the main text, and so have not moved these sections, but removed the subsection titles so that all of this content now falls under a single Section 2.2. The contents of section 2.2.2, which is only a single paragraph, detail the interpolation procedure used to oversample the radar range axis, which is important context for understanding the simulation results. The contents of section 2.2.3 include specification of the high-level radar parameters, spatial averaging scheme, and the uncertainty model, and again are important for understanding and interpreting the results presented in Section 3.

(ii) reduce their selection of Figures 5-12; e.g. by compressing the presentation of the simulated measurements and/or moving them to Sec. 2.2.3 or the appendix

These figures constitute the main results of the paper, which are the analyses of end-to-end DAR simulations performed on 5 distinct LES cases. We believe that the amount of detail presented is warranted given the scope of the study, and therefore do not believe that these figures should be compressed or moved within the manuscript.

***Specific comments:***
1.Two relevant contributions to this field are missing in the introduction: Lamer et al (2021), doi.org/10.5194/amt-14-3615-2021 ; Schnitt et al (2020) , doi.org/10.1175/JTECH-D-19-0110.1
We thank the reviewer for pointing these out. We have included references to these two publications in the introduction.

2. p.3, ll. 16 More details should be provided on selection of 155.5GHz channel as third frequency. Why was the channel at 167 GHz (Roy et al, 2020) moved to 168 GHz here?
Please see response to General Comment #4 above for the reasoning behind the 155.5 GHz channel. Given a minimum (155.5 GHz) and maximum (174.8 GHz) DAR channel location, there is freedom in choosing where the 3rd DAR frequency should go in the interval (155.5, 174.8). We chose the value of 168 GHz because it is the DAR frequency that minimizes the correlation in the PBL between the fit parameters $a_2$ and $a_3$ in Eq. (6) and (7) of the revised manuscript. We note that the difference in this correlation between 167 and 168 GHz is not large.

3. p.4, tab. 1: the following LES parameters should be added: vertical top height of LES simulation; longitude and latitude of domain center; considered time step; IWV variability within the domain. I suggest to indicate mean and standard deviation. IWV (or TCWV?) should be used consistently throughout the manuscript. I also suggest to quantify the variability of $\rho v$ and IWV in the respective simulations to give the reader a feeling on the expected variations of the respective moisture fields.

The requested values for each LES case have been added to Table 1.

4. p.4, ll.5: A clarifying sentence should be added on how the different horizontal and vertical resolutions of LES and MERRA-2 were accounted for when completing the LES columns. A reference and specifications to the used MERRA-2 dataset are missing.

We note that in the original manuscript, it is stated that we utilize one-dimensional MERRA-2 reanalysis data for completing the LES columns, and so the horizontal resolution difference is not necessarily relevant. In the revised manuscript, we have provided a proper reference for the MERRA-2 data set, and have added a sentence listing the horizontal and vertical resolutions for MERRA-2.

5.p.4, Sec. 2.2: also see major comment 5. The authors should highlight the advantages of their radar simulator compared to existing radar forward models.

We point the reviewer to our comments under major comment 5.

6. p.8, l.17: Which 'relevant variables' were chosen?

We thank the reviewer for pointing out this confusing phrase. All that is meant here is that the integrand of old Eq. 4 (now Eq. 1 in the revised manuscript) is summed at the discrete model points. We have removed the phrase "relevant variables" in the revised manuscript to make it clear that we're simply performing the discrete version of the integral defined in that equation.

7. p.9, tab.3: range indication for *dBZmin* and explanation for * in horizontal footprint line are missing.

We have clarified in the table caption that the minimum detectable reflectivity corresponds to the surface range. The asterisk in the horizontal footprint line was a typo in the original manuscript and has been removed in the revised version.

8. p.11, Fig.2: I suggest to add the reflectivity profiles of f0 and f2 to the right panels of (a) and (b), respectively. Considering a detection threshold of -34dBZ as given in Tab 3, a detection of the profile as shown in (b) below 2km does not seem possible. A clarifying statement should be added on how saturated profiles are dealt with in the retrieval, and how often saturation occurs in the 5 different LES cases. How many profiles were simulated in total? CFAD diagrams could give the reader an additional overview on the simulated radar measurements.

We first note that the point of this figure is to investigate in a "toy model" fashion the bias differences between the 2-frequency and 3-frequency DAR approach, and we specifically discuss in the text (Pg. 11 line 5-8 of the original manuscript) that this analysis is not performed with the full radar simulator, but in a simplified model to only highlight that these biases exist even for a "perfect" measurement. To clarify this point further, we have edited the second sentence of the caption to Fig. 2 as follows (new text in italics): "An ideal reflectivity measurement is assumed, with no multiple scattering, arbitrarily high range resolution *and measurement sensitivity*, and zero random measurement error."

The detection threshold is very important for the main simulation results in the paper, and is applied appropriately therein as can be easily seen from the reflectivity profiles in Fig. 3(d).

We're not sure what the reviewer means by "saturated profiles", but are assuming that they are getting at situations where the beam is heavily attenuated by liquid and water vapor. In the simulations, we account for this reality by removing any reflectivity observations below the detection floor, as explained

Finally, we believe that a CFAD-style analysis is well outside the scope of this toy-model-like investigation that is not part of the results section of the paper.

9. p.15, ll.11: the authors should clarify how exactly they chose the profiles that were sampled when applying intelligent scanning sampling strategy.

We thank the reviewer for pointing out that we did not explicitly state this criteria. We have added text to clarify this point.

Added Text page 14 line 3: "In this work, we assume that for each along-track pixel the antenna scans to the cross-track location with maximum liquid water path."

10. p.15, ll.11: Do the authors think that an intelligent scanning technique could be, in a realistic application, combined with adjusting one of the frequencies to overcome sensor saturation for the expected conditions?

It is an interesting comment and is something that we have thought about. There is no technical reason why this could not be done – however it would be somewhat challenging to know given some ancillary measurement (we envision a forward-looking radiometer) to know exactly which frequency, or pulse shape, etc. would be best suited for each target. Our guess is that the complexity (=cost) involved in going down this path would not be justified by the return on that investment. At this time if we can just avoid the clear-sky we would be happy. We leave this discussion out of the manuscript as it is not something that we are likely to pursue and is a bit distracting from the story.

11. p.16, Fig.3: figure caption for panel (d) is missing.

We thank the reviewer for catching that this caption was missing. We have included the panel (d) description in the figure caption in the revised manuscript.

12. p.17, ll.2; Fig. 4(c) (and 6,8,10d): The authors should add a clarifying comment how the $\rho v$ precision changes at different heights (e.g. lower BL below 3.9km in the GATE case), or why the specific shown height was chosen. I suggest to add a clarifying statement, or to add the precision at different heights to the respective panel.

The heights used for this precision scaling analysis are chosen such that they lie well within the average cloud/precip vertical extent, and therefore have a good amount of radar sampling. Of course, one could always choose a more pessimistic case near the cloud edge, and the degradation of this retrieval precision as a function of vertical position can be clearly seen in the error bars of the (a) panels for the respective cases/figures.

Added Text to Fig 3 (Fig 4 from the old manuscript) caption: "For each LES case, this profiling height is chosen to lie well within the average cloud and precipitation volume to avoid sparse sampling near the lower and upper boundaries."

13. p.24, Fig.13: This figure summarizes the results of the intelligently scanning method nicely. How do these results differ when applying the analysis to the nadir sampling strategy? I suggest to address this in the text to uphold figure clarity. The authors should add to their discussion whether the derived along track averaging distance is sufficient to resolve the expected horizontal water vapor variability.

As one would expect, the retrieval performance generally degrades when going from the intelligently scanned to fixed-nadir system, though it does depend on the specific case (e.g., in regimes with high horizontal homogeneity it has a small impact). The main impact in going to the fixed-nadir system is that the error bars increase in a manner that can be clearly identified from the panel (d) plots in figures 4, 6, 8, 10, and 12. Importantly, the biases are quite similar, are generally small, and can be identified readily from the panel (a) plots in those same retrieval analysis figures.

For clarity we have added the following text to the figure caption: "While the retrieval biases are nearly identical for the fixed-nadir case with large along-track averaging, the precision is degraded and can be assessed by examining the precision scaling plots in Fig. 3(d), 5(d), 7(d), 9(d), and 11(d)."

14. p.25, ll.18: I am wondering if the required fulfillment of the 200m vertical resolution systematically excludes certain conditions – _can the authors provide a threshold or typical cases in which this requirement is not fulfilled?

As the retrieval is formulated in the current work, the requirement of 200 m vertical resolution in the retrieval is by definition always fulfilled for the "in-cloud" retrievals. Unlike for passive systems, the vertical resolution of the radar measurement and retrieval are perfectly well understood thanks to the range-gating nature of radar. Of course, it will certainly be the case in some regimes and at some vertical levels that there is not enough differential absorption signal or measurement sensitivity to perform the 200 m retrieval at the required precision, and the results of this study give a quantitative understanding of this for different cloud regimes.

15. p.26, tab. 4: A column giving the surface temperature should be added.

This information has been added to Table 1.

16. p.26 ll.20: A reference for this statement should be added.

We have appended the following text in the revised manuscript and added a relevant reference for this statement: "due to the large Rossby radius of deformation".

Reference: Schneider, E. K., 1977: Axially symmetric steady-state models of the basic state for instability and climate studies. Part II. Nonlinear calculations. *J. Atmos. Sci.,* **34,** 280–296.

16. p.27, l.15: the authors should provide more information about the applied LWC > 0.01gm-3 threshold and the in-cloud temperature derivation for thinner clouds. How does this threshold impact the presented results?

The LWC threshold only applies to analysis of the model variables themselves, and not the DAR-inferred temperature profiles (i.e., varying this threshold will not change the DAR results at all since it is not applied to them). While the exact values for the mean model RH and T profiles will vary slightly with this threshold, the take-home point of the figure is clear - that estimating temperature based on knowledge of the water vapor density and an assumption of RH = 100% will often lead to an underestimate of the temperature, or low bias. Reasonable adjustments of this threshold will not alter this conclusion.

17. p.27: Fig 14b) is referenced before 14a).

In the revised manuscript we have edited the initial reference to this figure to simply state "Fig. 14" instead of "Fig. 14(a)", since this reference introduces the figure as a whole. Later we reference 14(a) to explain that these RH profiles are used as input to the derived temperature profiles in 14(b), which is the natural flow for the figure.

18. p.28, conclusions: based on their experience with the present analysis, I encourage the authors to add a concluding remark how their simulated system would perform in different conditions such as at higher latitudes.

We have added the following text to the end of the conclusions section: 'While this study chose the subtropical to tropical marine cloud transition to evaluate the potential capabilities of a spaceborne DAR we can speculate as to the expected performance in different cloud regimes. We expect two primary limitations. First, we expect significant degradation of the radar sampling due to attenuation in the most heavily precipitating storms (e.g. mesoscale convective systems, Hurricanes, and strong mid-latitude frontal systems. Second, relative to the subtropical ocean many land regions will have insufficient scattered cumulus to acquire significant samples to perform vapor retrievals even with a scanning system. On the other hand, we expect that DAR retrievals will be quite useful in the midlatitude storm track regions over ocean in regions of post frontal convection and stratocumulus which are frequently multi-layered and lightly precipitating thereby providing ideal targets. In addition, cloudy polar and high latitude scenarios will be particularly well suited to the DAR technique because of the relatively small amount of attenuation that the radar would experience.'